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A CONTRIBUTION TO LAMARCKIAN EVOLUTION.

Sexual Dimorphism in the Animal Kingdom; a Theory of the Evolution of Secondary Sexual Characters. By J. T. Cunningham, M.A. Pp. xi + 317; with 32 illustrations. (London: Adam and Charles Black, 1900.)

HOWEVER much readers of this work may dissent from the views of the author, there can be no doubt that the volume is worthy of the most careful perusal. For the first time since the publication of Darwin's theory of sexual selection we have been provided with a bold and intelligible attempt at explaining secondary sexual characters on Lamarckian principles, and although many of us may arrive at the conclusion that Mr. Cunningham has not succeeded in establishing his case, it will be generally admitted that he has discussed the problem, on the whole, in a more or less scientific spirit, and has supported his arguments by a body of well-considered and, in many cases, original observations, which make his book exceptionally valuable as a storehouse of facts.

The author, as is well known, belongs to that school of anti-Darwinian evolutionists which accepts the broad doctrine of descent with modification, but which denies the sufficiency of natural selection as the cause of species formation. In the introduction he re-states some of the chief difficulties and objections which have been urged, over and over again, by the opponents of Darwinism. An analysis of these objections, as set forth by Mr. Cunningham, will show that they resolve themselves mainly into the inutility of incipient stages, the dictum of Romanes that natural selection is a theory of the origin of adaptations, the inutility of specific characters, the failure of natural selection to account for the origin of variation and so forth. Students of evolution are so familiar with these much-discussed topics that we may be excused from dealing with them again in detail. It is ancient history that Darwin admitted "use and disuse" and the "direct action of external conditions" as factors of some value in the production of species. But he assigned a subordinate function to these agencies, and it is quite unfair to Darwin's position to state, as Mr. Cunningham does (p. 12), that "if once we admit this, selection becomes a secondary and subordinate character."

What concerns us most here, however, is not so much the destructive part of the present work, because the author, in brilliant contrast to many critics belonging to his school, has not contented himself with mere cavil or with the watering-down process which is rife among certain sections of naturalists who regard with horror any attempt at dealing with the species problem by scientific method. Mr. Cunningham has formulated his own views, and has applied them to the particular, and, we may add, absorbingly interesting, class of phenomena presented by animals with dissimilar sexes. These views are, as the author will admit, purely Lamarckian—in fact we might say grossly mechanical, since the secondary sexual characters are regarded as the direct result of mechanical irritation or stimulation (p. 37). It is needless to point out that this view is absolutely at variance with that held by selectionists. It is a doctrine which has been broached of late years to account for floral structures, and which, if we are not mistaken, has received but little favour from botanists.

In defining Mr. Cunningham's position as a Lamarckian, it is necessary to point out, in order that full consideration may be given to his views, that he has introduced a certain modification into that doctrine which he claims—and we think rightly—to be original. Lamarckism, of course, carries with it the admission that acquired characters are hereditary, and the author's attitude towards this question will be considered subsequently. But whereas the original, or proto-Lamarckian, or his modern successor, the neo-Lamarckian, never appears to have troubled himself very much about the precise period in the life of the individual at which the "acquired" characters were produced, Mr. Cunningham has laid it down, as the essential part of his amendment, that these characters only become developed (by heredity) at "that period of life and in that class of individuals in which they were originally acquired" (p. 37). He further postulates that, in order to produce such hereditary acquired characters, the stimulations must "be regularly recurrent," and their transmitted effect is then only developed "in association with the physiological conditions under which they were originally produced" (p. 41). In other words, he re-states Darwin's "inheritance at corresponding periods of life" and "inheritance as limited by sex" from the Lamarckian platform, and imposes a new restriction in the way of "physiological conditions" which are nowhere defined throughout the work excepting in the case of secondary sexual characters, where the supposed conditions are vaguely associated with the change of constitution accompanying sexual maturity.

From these considerations it follows that the external stimulus or irritation which (admittedly) can modify a part or organ of an individual during its lifetime, is only capable of producing modifications of specific rank when applied continuously, throughout many generations, at some particular, and at present undetermined, state of physiological activity. Supposing we admit, for the moment, that the author's position is sound for the only case in which such special physiological conditions are hinted at, viz., the period of puberty, then it follows that those mutilations which have been carried out through successive generations in many tribes of savages at the precisely critical period required by the hypothesis might, at any rate, be expected to show now and again a tendency to appear spontaneously in the offspring at that period. The evidence on this point is certainly against the author, and the case for Lamarckism in its amended form receives no more support by virtue of Mr. Cunningham's amendment than did the original Lamarckism from the consideration of such classes of facts. It is, perhaps, not going too far to say that the author's position is less tenable by virtue of his own restriction than that of the older Lamarckians, because the whole explanation of sexual dimorphism, from Mr. Cunningham's standpoint, is made to depend upon the action of external stimuli applied at the period of breeding, *i.e.* at sexual maturity.

It has already been stated that the author repeats, and therefore presumably gives weight to, the stock objection against Darwinism that that doctrine does not account for the *origin* of the variations which selection has to deal with (p. 30). True; but the writer of this notice is not aware that the selectionists have ever pretended that it did, or that it might be expected to. There have been supplementary hypotheses of variation, and there is reason to believe that variability or instability of form, if of advantage to a species subject to rapid changes of environment, might be seized upon and perpetuated by selection like any other character, structural or physiological. But this has nothing to do with the origin of variation as the result of physiological processes. On the other hand, if the theory of natural selection is considered to break down because it fails to account for the origin of variations (which at any rate are facts) what value can be assigned to a theory which *assumes* a special plasticity of the organism under particular "physiological conditions" at a certain period of its life without any adequate proof that such plasticity does exist, and which further *assumes* that external stimuli, acting upon the individual at that period, produce hereditary modifications of structure?

I hope I am not misrepresenting the author's views in pointing out how much in the way of assumption exists in them. It will, no doubt, be said by Mr. Cunningham that his method is logically sound and scientifically correct. Having come to the conclusion that selection is inadequate to account for the facts of sexual dimorphism, he has a perfect right to ask naturalists to examine the evidence which he considers to weigh in favour of his alternative explanation. He says (p. 42):—

> "I maintain . . . that theories of selection being found on application to the facts to be insufficient for their explanation, and the theory of the inheritance of acquired characters being found to harmonise with the facts, we are logically bound to believe that such inheritance does take place, at any rate until some other explanation can be found. I do not concern myself with the question how such inheritance can be produced, *it is a fact that the modifications are hereditary, and my object is to produce inductive evidence that they were determined by special stimulations*." (Italics the author's.)

The author's position towards the question of the inheritance of acquired characters may be paraphrased thus:—Secondary sexual characters appear to be, on critical examination, the result of direct mechanical action upon the individual; therefore we must believe that these characters have been so produced. The non-transmissibility of acquired characters is not an established truth, but only a belief (p. 15); therefore, in view of the foregoing conclusion, we must believe that acquired characters are transmitted.

In answer to this we can only repeat the statement so frequently made in reply to Lamarckian arguments:—All the evidence hitherto adduced in favour of the view that acquired characters are hereditary is either negative or ambiguous, and in cases where, on such hypothesis, the evidence might fairly be expected to be positive, it is also negative or ambiguous. The probabilities are therefore antecedently against Mr. Cunningham's explanation of secondary sexual characters, and we are justified in asking that his proofs shall be very cogent and convincing before we abandon a theory so probable *à priori* as Darwin's sexual selection in favour of a theory which is based on unproved principles. If the non-transmissibility of acquired characters is not an established truth neither is the opposite view, and the author—outside the domain of sexual dimorphism—adduces no new evidence in favour of this view. He says explicitly (p. 37) that he does not propose to prove that acquired characters are inherited, but a few paragraphs further on he lays down a statement of his own opinion which is tantamount to a declaration that such characters are inherited. It may be well, also, before attempting to deal with the detailed statements contained in the seven chapters following the introduction, to point out once again that the obvious adaptation of the parts of an animal to the life habits is no proof that the structural modifications have been produced by the habits. It may appear, *primâ facie*, that the structure is caused by habit, but this is no proof that it is so caused. The same result can be brought about by selection, and it is quite unnecessary to insist here that this is the essence of the Darwinian theory. But Mr. Cunningham is quite consistently Lamarckian in his inversion of the Darwinian position. Throughout this work the reader will meet with statements which remind us of the old Lamarckism—that such and such a structure *is produced by* such and such habit. In one illustration (p. 171) he even goes so far as to demonstrate that by moving a pen-holder coated with sealing-wax to and fro in hot water the softened wax forms a lamina above and below in the plane perpendicular to the plane of the motion, from which he apparently wishes us to believe that the crest of the male crested-newt "is due to the active movements of the male in courting the female in the breeding season"!

The special evidence which the author brings forward in support of his views is contained in some two hundred and sixty pages, divided into seven chapters, each dealing with the secondary sexual characters of some class or classes of the animal kingdom, from mammals downwards. It is impossible to deal with the various cases in detail, but a few typical examples may be selected in order to illustrate the position of those who, like Mr. Cunningham, regard habit as the cause of structure. The discussion of the sexual differences in man, in the first chapter, will be found particularly instructive. The hair on the face of the male is supposed to be "due to the stimulation of the growth of the hair by teeth or hands in the combats of mature males" (p. 49). That is to say that man's ancestors, by pulling each other by the hair of the face during their struggles for the female, developed a beard and moustache. But if mechanical irritation or stimulation of the hair follicles is the cause of increased growth of hair, why should the hairless condition of man's body, as compared with that of the apes, be attributed to the wearing of clothes (p. 52); the baldness of civilised races to the wearing of hats (p. 53), and the greater length of hair in woman to the wearing of lighter head-gear (p. 54)? Surely the hair follicles of a clothed animal are subject to more pressures and stimulations than in the naked animal? In a similar way, the author discusses, in this chapter, the origin of antlers, horns and tusks, which, being used for fighting, have, according to his views, been developed by pushes and stimulating shocks. The cheek prominences of the male mandrill are considered to have been produced by a similar process, and their furrows and colour are regarded as "inherited swellings and scars" (p. 58). The case for Lamarckism, as based on a study of antlers, is worked out at great length also in this chapter, this being a subject to which Mr. Cunningham has devoted special attention. The tusk-like development of the upper canines of the male musk deer is explained in these words:

> "The enlargement of a tooth is as natural a consequence of excessive use as is that of an antler, the pressure stimulating the papilla or pulp from which the tooth is developed" (p. 96).

The author adds, however, that the evolution of the musk gland of the male "is more difficult to explain."

Those who are familiar with Darwin's weighty body of evidence in favour of sexual selection derived from his studies of the secondary sexual characters of birds will turn with interest to the discussion of this subject in the second chapter of the present work. The excessive development of male plumage is, according to the author, the result of use—*i.e.* to erection by muscular action during display. But the most striking sexual differences among birds are not only due to development of plumage, but also to colour and pattern, and just on this point Mr. Cunningham's application of Lamarckism becomes most unsatisfactory:—

> "With regard to the coloration and markings of special plumage I have little to say. I regard them as due partly to the same excessive growth as that which increases the size of the feather, partly to the universal regularity and symmetrical repetition of marks, due to the rhythmical nature of growth processes, and partly perhaps to the action of the light from particular surroundings" (p. 109).

From this point of view, it is interesting to see how the author deals with the ocelli of *Polyplectron* (p. 114). Darwin, as is well known, attributed the dull ocelli of the female to partial transference of the male characters. Mr. Cunningham inverts this explanation, and suggests that the present condition of the female represents the original condition of the male. In other words, the ocelli of the male as at present seen are the female ocelli developed and enhanced by "use inheritance." The writer of this notice fails to see wherein this explanation offers any advantage. The duller ocelli of the female have, on this view, still to be accounted for. What direct action or external stimulus or impact can be conceived which is capable of producing regular patterns? The "action of light from particular surroundings" (even if we admitted such action) could not produce an ocellus, and if the "rhythmical nature of growth processes" is considered a sufficient explanation, this and all similar cases are at once removed from the discussion, and have no more to do with the author's Lamarckism than with the older Darwinism.

The whole treatment of colour and pattern throughout the work leaves no doubt that the author proposes to bring these characters within the domain of his amended Lamarckism. Among Amphibia, for example, the more vivid colouring of the male *Molge aspera* is attributed to the greater sexual excitement of the male, which causes, "through nervous stimulation, more active production of pigment in the skin" (p. 175). Among fish—a class to which Mr. Cunningham has paid special attention—the same explanation is offered again and again to account for the brighter colours of males. In those exceptional cases where, as in *Solenostoma* (p. 227), the female is the more brilliantly marked and coloured, the author is obliged to reverse the nervous excitement of the sexes. Perhaps the most instructive case dealt with under this division is that of the male dragonet, *Callionymus lyra*, because it is discussed at great length (pp. 199—214), and may be taken as a typical example of the author's treatment of the problem of colour as a secondary sexual character. The habits of the fish have, moreover, been most thoroughly observed and recorded by Mr. Holt at the Plymouth Marine Station. In summing up this case Mr. Cunningham concludes, as before, that the increased sexual excitement of the male at the breeding period causes, through nervous stimulation, an increased deposition of guanine and pigment in the skin.

> "That the marked excitement of the male dragonet's brain should cause increased nervous stimulation of the skin is not an extravagant supposition, and it is definitely supported by the fact that the blue bands, if not the yellow, are flashed out in more intense brilliance as the fins are raised in his amatory rushes. When the question is regarded physiologically, instead of merely from the selectionist's point of view, the significance of such reasoning as I have used cannot be ignored."

In this paragraph Mr. Cunningham takes up a position that is very frequently assumed by opponents of the selection theory, and from which the writer of this notice desires to take this opportunity of again expressing his dissent. The fact that the blue bands are flashed out as the fins are raised is surely an optical effect. I can hardly imagine that even the Lamarckian zeal of the author would lead him to desire that his readers should suppose that the "amatory rush" of the male caused an immediate and suddenly increased secretion of guanine! But apart from this, supposing we admit that he has discovered the true physiological explanation of the colour—and that given on p. 211 may be perfectly correct—wherein does this favour the Lamarckian and discredit the Darwinian explanation? It appears to be constantly assumed by anti-Darwinians that because they have found the physiological and histological mechanism of a colour they have thereby disposed of selection. We say that selection is capable of acting upon *all* characters, external and internal—whatever their physiological origin; and, in this particular case, if the male fish were capable at the breeding period of secreting guanine to such an extent and in such an arrangement as to produce a visible blue colour, we should say that there is here a character which is quite as capable of being enhanced and developed by sexual selection as is any other character of biological value to the species by natural selection.

In association with the hypothesis that colour is the result of increased deposition of pigment due to sexual excitement, it may be pertinent here to raise the question why, on Lamarckian principles, the increased secretory and excretory activity should take the form of colour at all. It can be admitted in a general way that increased nervous stimulus might lead to increased secretion and excretion generally. But there is no evidence of any kind brought forward in support of this—the evidence which Mr. Cunningham asks us to accept in favour of his view centres round *local deposits* either of osseous or horny matter, &c., in the case of mammals, or of pigment. Colour, as such, is meaningless from the Lamarckian standpoint, and we have a right to ask where is the "use" (in the Lamarckian sense) which accounts for the accumulation of such deposits in definite stripes and patterns in one part of a wing or feather. If it be suggested that the definite arrangement is the external expression of an internal law of growth, then the diversity of pattern among allied species remains unexplained; and, if we allow the direct action of light as a cause of pattern, we are invoking an improbable and an unproved principle. It must be presumed that the author considers his experiments on the coloration of the *Pleuronectidae*, to which he refers on p. 41, as a sufficient proof of "the direct effects of stimulations." At any rate this is the only instance specifically mentioned which has any direct bearing on the question of the action of light as a colour-producing stimulus. It appears to the writer, however, that the results which Mr. Cunningham made known in 1896 are open to another interpretation from the point of view of the selection theory, but the discussion of this point would be out of place in the present notice.

If the "nervous excitement" theory of colour is considered sufficient as regards deposition of pigment, the production of colour by purely physical methods, such as striation and lamination, has yet to be faced by the Lamarckian school. This aspect of sexual coloration is very lightly dealt with by Mr. Cunningham, although, in the case more especially of birds and insects, some of the most gorgeous male characters are due to such colours. It is, perhaps, hardly right to attribute unfairness to an author who evidently has done his best to give due consideration to the doctrine which he is opposing, but impartial readers of the present book who happen to be acquainted with the subject at first hand cannot fail to detect the anti-selectionist bias which here and there makes itself manifest in Mr. Cunningham's pages. Nowhere is this more obvious than in the fifth chapter, dealing with the secondary sexual characters of insects. The bias is no doubt unconscious, but when we consider that among butterflies, to which the author devotes the larger part of the chapter, this kind of colouring plays such a very important part, the treatment will be found, to say the least, disappointing by those who wish really to know what the Lamarckians have to say on this subject. The explanation offered is that the colours—pigmentary and physical not being separated—are caused by the direct action of light:

> "If we assume that variations in definite directions are excited by external conditions, in this case principally by light of different colours, the facts become intelligible in general," &c. (p. 243).
>
> "By exposure I mean the kind of light to which the surface is exposed, and I believe that in Lepidoptera the coloration has chiefly been determined by the quality of the light" (p. 245).

We do not believe that the author will make many converts to his amended Lamarckism by such explanations as these. When and how does the light act, and how can such action produce pattern? Is the pattern focussed on to the wing by some mysterious agency capable of acting as a lens, or how does the light of different "qualities" (? wave-length) get separated so that each component produces its proper chromatic effect? Upon what organic tissue can light produce such direct photo-chromatic action, and does such action take effect through the skin of the pupa, in which stage the colours, both pigmentary and physical, are formed in the rudimentary wing? It seems to the writer that, instead of the facts being made "intelligible in general" by invoking such strained explanations, they have been rendered hopelessly inexplicable from the Lamarckian standpoint For example, to take his own illustration, does Mr. Cunningham wish us to believe that the hind-wings of *Catocala nupta* have been exposed to red and black light for generations—if so, where is the evidence? Does he seriously believe that the mottled-grey of the fore-wings, which, as he admits, makes them "indistinguishable on surfaces of bark or rock" (p. 246), is better explained by the direct action of the light from such surfaces than by a straightforward admission of natural selection for the purposes of protection? If he does prefer this explanation it must be left for the readers of this review to consider whether any bias is displayed by the author of the work under notice.

In discussing the colours of Lepidoptera, the author begins with those cases in which there is mimicry combined with sexual dimorphism, the females being, generally speaking, the mimetic forms. His object in doing this is to bring into prominence the undisputed fact that in such cases the two sexes have different habits. His contention, of course, is that the habits have produced the differences between the sexes. But if we do not admit this—and selectionists will not accept this doctrine until some much stronger evidence is forthcoming—the whole force of the argument is lost, because the difference in habit is also precisely in harmony with the requirements of the selection theory. What is really wanted to make this argument of any value is the proof that in species which are *not mimetic*, but which are nevertheless sexually dimorphic, there is a sufficiently marked difference between the habits of the sexes to account for the greater brilliancy of the one sex. So far as the writer has observed, there is no marked difference of habit leading to different exposure, for example, between the male and female in species of *Colias*, *Anthocharis*, *Pieris*, *Gonepteryx*, *Hipparchia*, *Satyrus*, *Argynnis Hesperia*, &c. Yet in all these there is in most of the species a marked dissimilarity of sex. To narrow down the issue, why, on Lamarckian principles, should the male orange-tip (*A. cardamines*) have an orange patch on the fore wings, and the female *Colias edusa* a row of orange spots round the black border? Why should the male *Gonepteryx rhamni* be bright yellow and the female greenish-white?

In all cases where direct observation is wanting, or where there is no observable difference of habit between the sexes, the author assumes that there must be some such difference. Of course this is a very easy way of "explaining" things by Lamarckism or any other hypothesis; but as this method is freely indulged in throughout the book, and since Mr. Cunningham is nothing if not logical, it may be well to examine a little more critically his position in this respect. The kind of assumption to which reference is made is well illustrated, not only by the chapter on insects, where colour is chiefly dealt with, but also in other classes where structures of unknown use are possessed by the males. Thus, among birds, the males of *Chasmorhynchus nudicollis* and other species of the genus are supposed, without evidence from observation, to fight and to

> "seize their adversaries by the skin at the base of the beak, the attack being directed to the chin or the forehead, according to the species" (p. 145).

This is put forward as a "probable conjecture," because the male characters require such Lamarckian stimulations to account for their presence. Among chamæleons, the male, *C. Owenii*, has three slender horns which are not known to be used for fighting;

> "but the animals are known to be quarrelsome, and it is probable that the appendages are thus used" (p. 166).

In this case the author even allows his logic to go by the board, because he follows up this statement, which is only conjectural, with the remark:

> "Here again, therefore, we find that the existence of outgrowths corresponds to the impacts produced by definite actions, both being confined to the male sex."

That is to say, that a function which is conjectured to be probable in one sentence is treated of as an established truth in the following sentence. Examples of this kind might be multiplied, but the foregoing will suffice. Now the point which the writer of this notice wishes to urge is that all the arguments of this class are arguments of analogy only, and the weight which attaches to them is dependent on the antecedent probability of the hypothesis in support of which they are brought forward. Mr. Cunningham has, by implication, admitted the antecedent probability of Lamarckian factors; but if we do not follow him—and no selectionist will—much, if not all, the force of the analogies is lost, and we have a right to demand proof of actual connection between male structures and their use in every case described by the author. It is not to the point, in such cases, to urge that because in one species the male has been seen to use appendages or excrescences for fighting, therefore in other species with similar growths these must be used for the same purpose. The theory of sexual selection allows that male characters of the kind referred to may have been developed for ornament or as weapons of attack or defence, or for both purposes. But Lamarckism must have a definite mechanical cause for each excrescence or appendage, and the failure to discover such cause in any particular case is so fatal to the doctrine that most impartial readers of Mr. Cunningham's work will, on this ground alone, reject his conclusions. In one case (*Turnix taigoor*, p. 118) where the females are known to fight, and where there is no modification of structure corresponding to this habit, the author suggests

> "that the pugnacity of the females has but recently arisen in the evolution of the genus, but it may be that the wounds inflicted are not very severe" (p. 120).

Many other points in the volume under consideration are open to criticism, and, in view of the importance of the issues, demand notice. Reverting again to the chapter on mimicry, the author, in rejecting the obviously simple explanation offered by selection for protective purposes, says that

> "the theory of specific mimicry involves assumptions that have not been sufficiently realised. It assumes that birds or other enemies of butterflies are as precise in entomological discrimination as the human specialist" (p. 241).

This is hardly a correct statement of the case; the assumptions made by the theories of mimicry (Batesian and Müllerian) have been thoroughly well realised, and a body of evidence, both observational and experimental, has been brought to bear upon these theories, which Mr. Cunningham has either overlooked or chooses to ignore. But his own assumption, which precedes this statement, displays such an astonishing misapprehension of the whole subject that we can only come to the conclusion that the author has not really made himself master of the theory which he is endeavouring to overthrow. Having admitted that mimicking forms may belong to persecuted groups, he still suggests that, after all, the mimics may not owe their safety entirely to deceptive *appearance*, and he then goes on to say:—

> "Inedible forms, such as the Heliconidæ, the gooseberry caterpillar and others, are distasteful largely in consequence of the presence of the pigments which make them conspicuous. *Therefore when a mimicking form acquires similar pigments it probably likewise ceases to be palatable to insect eaters*, and would be equally unmolested even if it possessed no particular resemblance to a species of another family" (p. 241. Italics ours.)

It really appears from this as though the author believed that the mimicry extended to similarity of pigment, which is not only contrary to the truth, as established by the researches of Mr. Gowland Hopkins, but is in itself so improbable *à priori* that it is surprising that Mr. Cunningham should have committed himself to the statement.

Considered as a whole, there can be no doubt that this fifth chapter is the least satisfactory in the book. It not only contains misstatements such as the above, but its omissions show that the author has not made himself acquainted with the recent developments in this direction. As the Arachnida are not considered at all, the splendid observations of Mr. and Mrs. Peckham on the courtship of spiders are not discussed. The reference to Poulton's experiments on the influence of the colour of the surroundings on the colour of the larva and pupa, as an illustration of the modifying action of the "quality of the light" (p. 231) upon the colour of the wing of a butterfly, is a false analogy. The statement (p. 248) that "the beauty of a butterfly's wing is equally visible whenever the insect flies" does not convey the whole truth, because in many of the most gaudily coloured males the full splendour of the colour is only seen from the front aspect, *i.e.* facing the insect and looking across the surface of the wing, the aspect that would be first seen by the female if the male were flying straight towards her. Nowhere is this iridescent colouring better seen, by the way, than in the male *Hypolimnas bolina*, figured by the author on p. 233.

Although there can be no doubt that Mr. Cunningham's plea for Lamarckism and the inheritance of acquired characters is on the whole very ably supported, it will not fail to strike many of his opponents that his anti-selectionist zeal carries him too far in some of his most fundamental arguments. Like many of the recent critics of Darwinism, the author now and again reads into the theory of selection certain deductions of his own, which he then proceeds to demolish without considering whether his deductions are legitimate or not. For example, he considers it an objection to the Darwinian explanation of flying mammals (p. 15) that

> "the variations in the condition of the skin in animals that do not fly or take long leaps through the air are not such as to justify the belief that these variations would make any difference in the struggle for existence when long leaps became necessary."

That is to say that other animals besides those that do fly might be expected to show, on occasion, the raw material, so to speak, for potential flight. This is a kind of argument which has constantly been used by anti-Darwinians. Why, because a certain species has been enabled to develop such and such a useful character, have not similar characters been developed in other species to which they might be equally useful? It appears to be again necessary to point out (1) that it is not part of the Darwinian theory to suppose that every species is capable of varying *in every possible direction*; (2) that modification in the direction of flight may not have been possible in the species which "do not take long leaps through the air"; and (3) that such species have, no doubt, survived by the development of some other method of escape or defence which is quite as effective. Arguments of this sort might equally well be urged against the Lamarckians. Why, for instance, do not the flying lemurs, squirrels and foxes, &c., fly as well as bats, seeing that the membranous expansion of skin is constantly being used for gliding through the air? The writer has no desire to press this point against the author of the present work.

Then, again, Mr. Cunningham insists frequently that his (Lamarckian) explanation accounts for unisexual inheritance, while sexual selection does not. I believe that I shall not be singular in declaring my inability to see any force in this argument. Inheritance, as limited by sex, is a fact. Mr. Cunningham says (*e.g.* on p. 252) that this limitation is due to the "stimulations" having been applied to one sex and not to the other. But even if this were the true origin of the male characters there is absolutely nothing in the Lamarckian explanation which accounts for their non-transmission to the female. All characters, whether secondary sexual or not, must on either hypothesis (Lamarckian or Darwinian) originate in an individual, which individual must—it is needless to say in the case of bisexual animals—be either male or female. Why, then, should there ever be any blending of characters at all? According to Darwin's theory, the male characters originate through the selection of spontaneous variations by the females; according to the amended Lamarckism, the male characters are, as it were, hammered out of the male by stimuli applied at the period of sexual maturity. Why should this latter view be supposed to account for the limitation of the male characters to the male, and the former view (Darwin's) to fail? Or, to put the case in another way: Why should characters impressed mechanically upon the male during a particular period of his life be hereditary, and characters arising by spontaneous variation at that period not be hereditary? If the author had contented himself with the acknowledgment that unisexual inheritance required further explanation, both schools of biologists would agree. The selectionists might even have suggested an answer on the lines laid down by Wallace—that the female, being in most cases in greater need of protection and concealment, had had any tendency to inherit the male characters eliminated by natural selection. The author will no doubt reject this explanation; but whether he does so or not, it is manifestly absurd to claim for Lamarckism a fictitious superiority as regards the explanation of unisexual inheritance. The advantage seems to the writer to be distinctly on the side of the Darwinians.

If, as we believe, this latest attempt to reinstate Lamarckian evolution is unsuccessful, its failure is due to the inherent weakness of the doctrine rather than to the treatment which it has received in the present volume. It was an excellent idea on the part of Mr. Cunningham to test this theory by applying it to secondary sexual characters, but it does not appear that the light shed by Lamarckian assumptions is in any way clearer than that offered by the theory of sexual selection. There are, confessedly, difficulties in the way of both theories, but those offered by the Lamarckian doctrine are certainly the more serious. The writer, at any rate, has come to the conclusion that few biologists in this country wil accept the mass of material offered in the present volume as "inductive evidence" in favour of the heredity of acquired characters. Most of the cases discussed are equally well explained by the theory of selection—some cases are better explained by this theory. Assumptions and deductions have to be made by the supporters of both theories; those required by the Lamarckians seem to be much the less warrantable. It may be fairly said that if Lamarckism breaks down in its application to such characters as those dealt with, its career as a working hypothesis has ended fatally. It would certainly be interesting to know whether the author limits his Lamarckism to the production of secondary sexual characters, or whether he regards his amended form of the theory applicable to *all* specific characters. Limitation hardly seems possible logically, because any stimulus or impact of any kind whatever must (on this hypothesis) produce its effect if applied at the right stage of physiological activity. The author, if he accepts this wider application, will thus have narrowed down the issue between the two schools of evolutionists to the simple question whether hereditary modifications can be produced by such means. We believe the answer will be in the negative, but if this wider view is not accepted, then Mr. Cunningham must show cause why the modification should be restricted to such characters as those which he has so ably discussed in the volume which has been considered in this notice. R. MELDOLA.